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The Recombinant DNA Controversy: The NIH Viewpoint

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Revelation from human history takes time. And, as the gospels have shown us, both strong belief and the opportunity to work it out as a community also are helpful. This volume should help all of us weigh what we respectively believed and did during the recombinant DNA controversy, and judge the appropriateness of these actions with hindsight. A second and more important purpose is to consider not so much what we did as what we are to do in the future. This includes the future uses of recombinant DNA technology as well as the manner in which we should handle other controversies of a similar kind.

It is my assignment in this chapter to cast reflections from the many-surfaced mirror of the federal government. What did it do? What did it seem to do right—or wrong—and why? In the limited space available, I would like to state my views on some things the government did right, and where it might have gone wrong. Of course, I will have some biases, and this is the first occasion since leaving the NIH directorship that I have had to air them.

NIH as Lead Agency

In my opinion, one of the good moves of the federal government was that it let the National Institutes of Health carry the principal federal responsibility in the controversy.[1]

In 1977, one famous scientist wrote, "I consider it a true calamity that the agency dispensing nearly all the federal funds available for biological and biochemical research, NIH, has become a party in the debate."[2] Elsewhere, he opined, "the National Institutes of Health have permitted themselves to be dragged into a controversy with which they should not have had anything to do," and, "our time is cursed with the necessity for feeble men, masquerading as experts, to make enormously far-reaching decisions."[3]

Another critic wrote in the same period, ". . . an ethical conflict of interest arises when it [NIH] is entrusted to set up guidelines to regulate the very research it is committed to promoting."[4]

I agree with the appearance of a conflict of interest. It was unavoidable. It was bothersome all the way. One of the most important lessons to be learned about controversy over use of high technologies, however, is the absolute requirement for expert opinion. The most informed experts very often will include those using or promoting the technology, and so there will be an appearance of conflict of interest. The art of solving this kind of problem lies in the manner in which one joins the experts with the other parties at interest.

The NIH had at least five distinct advantages that made it the agency of choice for establishing guidelines and providing the focus for federal activities concerning recombinant DNA:

1. NIH originally had been asked by the scientists involved to help. In the often-cited letter to *Science* of 26 June 1974 to the director of NIH, eleven scientists acting for the Assembly of Life Sciences of the National Research Council and including some of the key molecular biologists who were to attend the Asilomar Conference requested NIH to start a program to evaluate the biological and ecological hazards, to consider procedures to minimize the spread of recombinant molecules, and to devise guidelines for investigators.[5] NIH therefore had the confidence of the scientific community most directly involved.

2. NIH was funding far more of the research involving recombinant DNA technology than was supported by any other source. It therefore had the "clout" to enforce adherence to guidelines if that became necessary.

3. The most important advantage of NIH was that it was there—staffed, integrated into government, and ready to go. The recombinant DNA controversy needed to be dealt with "on-line" and within the existing framework of government. It can be a grievous error to assume that dilemmas involving profound questions about science should automatically require new, untested solutions. In the first three to four years of the controversy, I believe it would have meant chaos had this problem been handed to a commission made up of busy citizens, no matter how distinguished, who could give it only part-time attention.

4. Among all federal science agencies, NIH had a unique feature

whose essentiality in such a controversy was not immediately recognized. This feature was the great size and quality of its intramural research program. The presence on the NIH campus of many experts in the techniques in question—scientific peers of those in the extramural community—made it possible for NIH to weather the storms that blew up around the speculative hazards and the threats to scientific inquiry inherent in the crisis. From among the staff I quickly assembled the NIH "Kitchen-RAC," which counseled and crafted solutions to endless problems as this complex and lengthy transaction proceeded. As director, I spent a third to a half of my time on recombinant DNA in 1976 through 1978. This was but a small fraction of the total NIH person-hour expenditure. I would have wasted those hours of mine but for the dedicated and talented scientific and administrative NIH people always at hand. Praise of these persons is one of the neglected choruses in the recombinant DNA epic.[6]

5. NIH was a science agency without formal regulatory experience or authority. Had it been an agency so endowed, such as, for example, the Food and Drug Administration (FDA), the Centers for Disease Control (CDC), or the Environmental Protection Agency (EPA), it could not have kept the setting of standards and their revision out of the chilling grip of conventional regulation. This would have involved the tedious, stepwise processes specified under the Federal Administrative Procedures Act. Every decision arising from the stream of new knowledge would have had to be diverted into the *Federal Register* for publication and comment. The risk of slowing the evaluation of the science into a sludge of unfinished experiments and unsatisfied hypotheses was too great. Rather, with the concurrence of superiors in the Department of HEW, I decided we should take advantage of the previously established practice of NIH directors to impose certain conditions upon scientists as guidelines. To this we would add the specifications of the Federal Advisory Committee Act protecting public access to the decision-making process. We would refrain from the formal *Notice of Proposed Rulemaking* which would have placed us in lock-step with the regulatory process. To be sure, well-informed critics such as former FDA General Counsel Peter Hutt took the NIH to task for its unseemly amateur performance as a regulator,[7] but we were determined to walk the narrow edge of the abyss until a special procedure for evolving the guidelines, consonant with the pace and scale required to synchronize experiments in scores of laboratories, could be established. As it turned out, this took two years of departmental negotiations and public hearings.[8]

Ecumenical Executive Agencies

Once harnessed, all the government executive agencies, research and regulatory, worked harmoniously.

In the beginning, there were at least four government agencies supporting recombinant DNA research (National Science Foundation, Department of Agriculture, Veterans Administration, and NIH). These agencies, it quickly became apparent, would need to agree upon a single set of standards. Moreover, there were a half-dozen, including FDA, EPA, CDC, the Occupational Safety and Health Administration (OSHA), the National Institute for Occupational Safety and Health (NIOSH), and the Federal Transportation Agency, which believed their authorities permitted them some regulatory authority over the products of such research, and perhaps even the laboratory experiments. As soon as we had guidelines ready for promulgation, I obtained the agreement of David Mathews, then Secretary of the Department of Health, Education, and Welfare (DHEW), to urge President Ford to convene an interagency committee. It was to include all of the above federal agencies and others which had special interests in the problems, including the Departments of Justice, Defense, and Commerce, the Council on Environmental Quality, and the Office of Science and Technology Policy (OSTP).

Months went by with no issuance of a presidential order. Disagreements among agencies over jurisdiction increased. Finally, Senators Kennedy and Javits issued an open letter demanding that the Interagency Committee be formed. The White House acted and the committee, which I chaired, got down to business in November 1976. Fairly quickly it achieved three objectives:

- All the research agencies (despite some sacrifice in autonomy) agreed voluntarily to adhere to one set of standards and to support a single locus of interpretation (NIH).
- All the regulatory agencies submitted to a common examination of their enabling statutes and agreed that none had clear authority to regulate, except in limited areas.
- The committee concluded that a new law would be both required and desirable to assure that the NIH guidelines were followed in all similar research in the private sector.

So a prescription for a model statute was developed. It included preemption of all other standards by federal ones and a sunset clause. This was forwarded to Joseph A. Califano, Jr., then Secretary of DHEW. His general counsel drafted a bill to be sent by the administration to Congress for action to create the new law. When the draft was sent through the Office of Management and Budget for clearance, last-minute anxiety on the part of one federal agency about the Secretary of DHEW having authority over the experiments conducted by its scientists threatened to send us all back to the drawing board. The government bill survived to be introduced. It was quickly lost from sight, however, as I will relate later.

All the government agencies also were given liaison membership in the Recombinant DNA Advisory Committee (RAC), where decision making under the guidelines proceeded. An Industrial Use Subcommittee, under the chairmanship of Gilbert Omenn of OSTP, was formed within the Interagency Committee to consider concerns raised by NIOSH and OSHA about the risks of industrial scale-up for recombinant technology.

There was considerable concern over turf within the federal bureaucracy, recombinant DNA being a matter of high press and public interest. The mandates of regulatory agencies do not allow them any rest if any possibility exists of their being considered derelict in a responsibility. The Interagency Committee allowed anxiety to be relieved by augmented communication and frequent discussions. In addition to maintaining a desirable amount of ecumenical spirit, this committee also had the virtue of being in place and ready for immediate convocation in the event that one of the hypothetical hazards did materialize and national resources needed to be mobilized and coordinated.

Other Struggles for Jurisdiction

The controversy over the use of recombinant DNA technology was not limited to Washington. Indeed, in late 1976 and 1977, the state of New York and several communities such as Cambridge, Massachusetts, and Ann Arbor, Michigan, were the sites of vigorous debates. From them arose pressure for enactment of new local or state laws, threatening a Balkanization of the scientific effort through differing regulations for conduct of laboratory experimentation. Such situations are rare in modern history and pose an unusual threat of disruption of scientific activity. This was the background against which the federal Interagency Committee conducted its work and the RAC proceeded to consider how to revise the original NIH guidelines that set the rules for all federal supported laboratories after 23 June 1976. It was the extension of these rules to all laboratories, whether receiving federal support or not, that partly maintained the controversy. Disagreement also existed as to whether these guidelines were too strong or too weak. For many, the interests lay in the procedures the scientists were ready to follow in revising the rules, particularly if they intended to relax them. The controversy became hotter when the Ninety-fifth Congress convened in January 1977.

Congressional Caution

The Congress, despite the introduction of more than a dozen bills and intensive hearings on the subject, eventually refrained from enacting a statute to control laboratory experimentation with recombinant DNA.

The activities of the Congress relative to recombinant DNA merit a more thorough and thoughtful analysis than is possible in this chapter. One would like to explore more carefully the various motives of the legislators and their staffs that impelled them to propose legislation. Needing detailed description, too, are the hazards of drafting statutes to restrict scientific freedom in a single, highly technical area. The play of forces that ultimately led to a stalemate, no bill actually coming to a vote in either house, is a theme for several essays. There were not only conflicts over passage of new legislation but also over interpretation of old laws to achieve the same purposes. Of particular relevance here were the attempts to make Section 361 of the Public Health Service Act (42 U.S.C. 264) the basis for nationwide regulation of recombinant DNA research. This little-used section permits the Surgeon General to take steps he deems necessary "to prevent the introduction or spread of communicable disease," a potential hazard of the use of recombinant DNA technology. For this there was a notable absence of proof. Many members of Congress, as well as the secretary and the general counsel of DHEW, and the Surgeon General joined NIH in opposing use of Section 361. There was a general feeling that if Congress wished to regulate laboratory experiments in biology, the members should stand up and be counted.

A rereading of the bills submitted reveals, amid the boilerplate, some intimate glimpses of tensions experienced by the congressional sponsors. Some of the "Whereas's" were followed by dire predictions, others by acknowledgment of wondrous benefits to accompany any hazard. The kinds of fines and penalties to be assayed showed how clumsy and unrealistic are the provisions of statute for governing this kind of human activity.

The most provocative piece of legislation proposed was S. 1217 (Amended), introduced by Senator Edward Kennedy in July 1977. The initial bill, which he had submitted in April, was the administration's minimal proposal prescribed by the Interagency Committee.

I supported this original "administration bill." It provided for federal preemption of any local regulations. Such preemption is always a controversial matter. It is consonant with the essential universality of science, however, and—something more practical in the recombinant DNA affair—it was in keeping with the absence of imminent danger to any particular community. The sunset provision of the administration bill also was a comfort. Any legislation over so mobile an activity as scientific research should have limited life expectancy. As in the other bills, the penalty clauses were harsh and foreign to scientific research; but they were a bearable price, if we had arrived at the need for federal legislation in order to allow experimentation to proceed.

But S. 1217 (Amended) also contained a new Title XVIII establishing a National Recombinant DNA Safety Regulatory Commission. The body would be serviced by DHEW but not clearly answerable to the Secretary. Of

its eleven members, six were never to have engaged in molecular biology. Thus, a new administrative creation would be established to supply the consensual requirements, the majority of participants to be inexpert in the subject matter. The commission would set the rules (as neatly promulgated regulations), license laboratories, and monitor compliance. Any extra time left over during the periodic visits of its members to Washington was consigned to a thorough analysis of "all the basic, ethical, and scientific issues involved."

The recombinant DNA controversy excited the natural tendency to assume that dilemma involving profound new questions should automatically be exposed to new, untested mechanisms for coping with them. Fortunately, the maturity of our political system—or the stubborness of its traditions—forced this complex problem to be engaged first within the existing framework of government. The recombinant DNA issue would have been gravely confounded by immediate convocation of one or another of the ad hoc commissions contained in numerous bills and articles stimulated by the controversy. One proposed that the Vice President chair the proceedings; another invoked a science court. One quickly learns in public service that there is no such thing as "immediate" chartering, staffing, and convening of a commission for any purpose, let alone preparing the first attempts at statutory regulation of complex laboratory experiments in biology.

One evening during the legislative furor over recombinant DNA, I was summoned to the bedside of Congressman Olin Teague at the Naval Medical Center. "Tiger" Teague was chairman of the House Science and Technology Committee. He sought a full explanation of the new biotechnology and an opinion about possible effects of pending legislation upon the progress of science. He questioned me for over an hour and listened carefully to the answers. Later, I heard how Teague had made sure that the House bills to regulate recombinant DNA by statute were sequentially referred to his committee for a long and thorough hearing. In so doing, he provided a way for tempers to cool and protected us all from hasty passage of laws that would have been injurious. When NIH and DHEW devised ways for industrial and other private-sector laboratories to comply voluntarily with the NIH guidelines, the pressure for legislation was nearly gone, and has not since been revived.

Imaginative Structures

Government was not devoid of imagination in creating new administrative structures to permit the public a role in the recombinant DNA controversy. The "second generation" RAC was perhaps the outstanding case in point. Reorganization of this committee was a concession demanded by Secretary Califano for permission to release the first major revision of the original

guidelines in December 1978. The original RAC, formed in 1975, had contained only scientists, nearly all of them molecular biologists. A political scientist was then added and somewhat later an ethicist came aboard. We initially employed the NIH Director's Advisory Committee, suitably augmented with a broad selection of scientists and laymen, as a second tier for review. It was the traditional organization selected by the Congress for the greatly expanded public support of basic research that commenced in the early 1950s.[9] The system is based on initial peer review (study section) followed by oversight of a group including laymen (advisory council) and has proved admirable for determining the allocation of resources for research. It is not effective for supervision of technical guidelines requiring continuous and rapid evolution.

DNA technology was exceedingly complex material, heavy going for laymen, but also for scientists from other disciplines. Procurement of advice and approvals in two stages created confusion and added intolerable delays. Hence, the RAC was changed to collapse review into one group. Its membership was composed of one-third molecular biologists, one-third scientists who were experts in genetics, microbiology, and other fields directly applicable to recombinant work, and one-third experienced in related matters such as public health, law, consumer affairs, or public policy. I have observed, particularly in the technical consensus exercises we established at NIH in the same period,[10] that when the nonexpert is not able to comprehend much of the detail, his public-policy role may better be performed in the midst of the experts. Here, at least, the lay person can observe the experts to see if they appear to be listening to each other and paying some attention to the evidence.

The new RAC, like the first one, remained advisory to the NIH director, who had the responsibility and authority for revision of the guidelines. Some of the scientists were alarmed when the new RAC was put into position,[11] but their fears of a shift of governance from the scientific to a political sphere did not materialize. I believe the new RAC was one of the most useful cultural innovations—for combining expert and nonexpert opinions about science—to emerge from the recombinant DNA controversy. Its success has been due to careful selection of members, to their generally enlightened individual performances, and above all, to the guidance of the chairmen. These have been Jane Setlow, a molecular biologist, and Ray Thornton, a lawyer and university president and once the congressman who conducted the Science and Technology Committee hearings on recombinant DNA fostered by "Tiger" Teague.

International Affairs

We were careful that NIH should not confuse its predominant place in the world of biological research with a mandate to determine the regulations

that would govern recombinant DNA research in the rest of the world. The sovereignty of each nation over its scientists was not a debatable issue. The role of the United States was both a delicate and influential one. Initially, we considered it likely that there would be less controversy in most other nations and that conditions outside the United States might well favor migration of our scientists to areas more congenial to the experimentation. It was certainly true that the extremes of reaction were observed in America and that in some countries there was little or no concern. Nearly all of the countries with advanced science and technology did adopt rules, however. The United States, Canada, and the United Kingdom were the first to have explicit guidelines. The major counterpart to the RAC proved to be the British Genetic Manipulation Advisory Group (GMAG), which set out to construct rules for the United Kingdom following the Ashby report (see Chapter 4). The NIH guidelines were the first of numerous different national rules to be released. Care was taken to distribute them abroad. More than forty countries were sent the guidelines by diplomatic pouch on the day of release in 1976, accompanied by a mission alert from the office of then Secretary of State Henry Kissinger.

There was continuous follow up. Among my papers are special travel diaries entitled "The Recombinant Odyssey." They summarize my numerous visits to scientists and officials of countries which included, among others, Great Britain, Germany, Canada, Holland, Switzerland, the People's Republic of China, Japan, France, Sweden, Finland, Italy, and the Soviet Union, as well as the European Economic Community in Brussels, to explain what NIH was doing and to learn how the other nations were attempting to regulate the research. I recall paying an early visit to Munich to see the new chief executive of the European Science Foundation (ESF). The late Franz Schneider informed me that he was sure the ESF would adopt the U.K. rules. When I crossed the city to Professor Feodor Lynen's Max Planck Institute, however, I found a scientist busy translating the NIH guidelines into German.

After my first visit to the British Medical Research Council and to GMAG, I realized that we were likely to achieve a kind of parity with the United Kingdom on containment rules despite a completely different mode for achieving them. The British proceeded to develop common law, case by case. The United States specified detailed rules in a veritable Napoleanic Code. The British met in closed rooms, protected by the Official Secrets Act. The United States opened the doors to everyone and compiled a massive record of the proceedings.

We worked hard to assure conditions for maximum communications and consensuality among all the users of the "new biology." Officials of the European Medical Research Council, the European Community, the European Molecular Biology Organization, and the Committee on Genetics of the International Council of Scientific Unions were often present at the sessions of the RAC and the Director's Advisory Committee, which were al-

ways open meetings. One of two major meetings which the NIH sponsored in 1977 to help clarify scientific knowledge on which the guidelines were based was held in Ascot, England, so that British and continental scientists could more easily attend. The private sector, including early industrial users of the technology, and public interest groups much concerned about the activities of the former, were also tied into the loop of communications.

In fact, the NIH Office of Recombinant DNA Activities, headed by William Gartland, became the primary communications center in the world concerning recombinant DNA during the late 1970s. We also persuaded the Office of Management and Budget to let us start a new journal, the *Recombinant DNA Technical Bulletin*, to carry the actions of RAC and scientific communications around the world. The safety manual and other advisories compiled under Director of Research Safety Emmett Barkley's direction, was another of the many technical aids devised to help standardize certain practices here and abroad. The course of GMAG in Britain and the rules eventually adopted by other countries are described by Keith Gibson in Chapter 4. Most nations have adopted basically the NIH guidelines, which have remained commensurate with those of the United Kingdom.

Major differences in national standards would have precipitated a chaotic situation throughout the world—as would different regulations in the municipalities or states in the United States. Indeed, had guidelines of grossly uneven character sprung up within countries, or among them, the new biotechnology industry based on recombinant DNA methods conceivably would have risen in the Third World, or on ships "beyond the twelve-mile limit" in the fashion of gambling casinos.

OTHER STEPS

There were many other decisions and actions for the government to take about the uses of recombinant DNA technology. Each agency decision had to be shaped so that it could make its way past the checks and balances built into the federal process. For example, there was the decision to be made about NIH-DHEW policy on patenting inventions derived from the use of genetic recombinants. Opinions were solicited and a decision made acceptable to the Secretary. Shortly thereafter, I found myself before Senator Gaylord Nelson of the Committe on Labor and Human Resources, who had strong views about patenting anything discovered through use of public funds. Later revisions have considerably liberalized the policy he and I were discussing.

There was the decision to be made about how to return the authority for using the guidelines to the institutions where I felt the responsibility belonged. The composition and function of the institutional biosafety committees (IBCs) was another important exercise in political compromise. One

of the most signifiant moves was the determination of how the RAC might review voluntary submissions of proprietary data from private sector laboratories interested in scale-ups. The RAC's eventual willingness to do this removed the last powerful thrust for legislation. The guidelines no longer require such examination. While they did, it was necessary to persuade the RAC regularly to continue this service, for it was an exercise which annoyed many of the members.

It will be impossible for some of us to forget the problems engendered by compliance with the National Environmental Policy Act. The first Environmental Impact Statement on hypothetical risks of laboratory experimentation became a nightmare before it was accepted. Yet it proved invaluable in opposing the injunctions against experiments that were sought in the federal district courts. The record maintained by NIH from the inception of its role contains more history of the roles pressed upon the government and the manner in which they were played.[12]

The Future of the RAC

Although we have learned that the probabilities of harmful creation from using recombinant DNA technology are much less than some believed in 1975, no one can assign a zero probability to harmful effects now. No one should pretend we have absolutely no further need for community guidance or for continuous evaluation of such powerful technology. It is, however, reasonable to ask whether the risks have not narrowed to those already assigned to the common vectors and hosts, so that special containment precautions for gene engineering may now be wasteful. The problems that remain to be dealt with—such as release of recombinant organisms or plants into the environment, evaluation of the numerous products of recombinant genes, or the effecting of changes in the human genome by new techniques—are still there. But so are agencies and other institutions to cope with the regulatory aspects of most forseeable problems. If some of the federal regulatory forces appear to be weaker than in 1975, it would be very unwise to compensate for this by forcing NIH to assume regulatory roles it has justifiably resisted for so long.

There is a continued need for full communication and critique in the use of recombinant DNA technology. It is the key expression of the continuity or universality of science. One would not want to abandon the network of Institutional Biosafety Committees and the RAC, which lies at the center, until they clearly no longer serve a useful function.

It is my view that we are not finished with practical scientific questions and ethical issues related to gene splicing. These will not be the stuff of conventional regulation, and they will require a proper place or places for the human community to debate and resolve them.

As the rules for conventional laboratory experiments inevitably slide toward the status of guidance, and as broader policy problems replace detailed analysis of experimental protocols on the menu for its consideration, we should think about construction of a "third generation" Recombinant Advisory Committee.

I suggest it might have these features:

- Be designed to fill the combination tasks of the present RAC and Interagency Committees
- Be responsible to a cabinet officer, the most appropriate still being the Secretary of Health and Human Services
- Continue to be serviced by NIH, but with broader contributions from other agencies so that the collective aspect of the future enterprise will be stressed and facilitated
- Continue to have a distinguished chairman, from the nongovernment sector
- Continue to have a majority of expert scientists among its members, but a total composition tailored to reflect the problems anticipated in the next few years

Post-Game Critique

I have described a number of things that the federal government did during the recombinant DNA controversy. Having been at or near the center of those actions, I am not the one to judge each step or to assign a mark for the overall performance.

Such an assessment should be undertaken, however, for high science will confront big government again with similar dilemmas. We need a clear understanding of what was done and why. The design of the federal government is such that the public interest in technologies can be served without impairing the effectiveness of the scientific endeavor. This was the major civics lesson to emerge from the recombinant DNA controversy. It was difficult, however, to maintain the proper balances, and one should not assume that the system will never fail.

Notes

1. The National Institutes of Health (NIH) is an agency in the Public Health Service which, in turn, is part of the U.S. Department of Health and Human Services (until 1977, the Department of Health, Education, and Welfare). NIH is responsible for more than half of the federal support to the universities for scientific research and development. In addition, NIH has an "intramural" research program, located principally in Bethesda, Maryland, which is the largest biomedical research institution in the world.

2. Erwin Chargaff, "Uncertainties Great, Is the Gain Worth the Risk?" *Chemical and Engineering News*, 30 May 1977, pp. 32–35.
3. Erwin Chargaff, "On the Danger of Genetic Meddling," *Science* 192 (4 June 1976): 938–40.
4. Francine Simring, "The Double Helix of Self-Interest," *The Sciences*, May/June 1977, pp. 10–27.
5. Paul Berg, David Baltimore, Herbert W. Boyer, Stanley N. Cohen, Ronald W. Davis, David S. Hogness, Daniel Nathans, Richard Roblin, James D. Watson, Sherman Weissman, and Norton Zinder, "Potential Biohazards of Recombinant DNA Molecules," letter to the editor, *Science* 185 (26 July 1974): 303.
6. The daily menu for the "Kitchen-RAC" (named after the parent NIH Recombinant DNA Advisory Committee) was usually prepared by Joseph Perpich, associate director for program planning and evaluation, and Bernard Talbot, special assistant for intramural affairs. Perpich's combined medical and law degrees, plus a clerkship with Judge David Bazelon and time on the staff of Senator Edward Kennedy, enabled him to provide me with invaluable advice on meeting both legal responsibilities and political objectives. His specialty training in psychiatry also came in handy. Both an M.D. and a Ph.D., Talbot was the perfect antidote for pejorative views on productivity of government employees. His Stakhanovite work habits enable him to produce mighty drafts and redrafts of revisions of the highly technical guidelines in response to endless commentary and pressure for alterations. Other invaluable contributors were Emmett Barkley, director of the office of research safety; William Carrigan, editor of the NIH papers on recombinant DNA; William Gartland, director of the office of recombinant DNA affairs at NIH; Susan Gottesman, a scientist in the laboratory of molecular biology in the National Cancer Institute and a member of the NIH Recombinant Advisory Committee; Joseph Hernandez, an attorney and member of our division of legislative analysis; Malcolm Martin, a virologist and molecular biologist from the National Institute of Allergy and Infectious Diseases (NIAID); Richard J. Riseberg, NIH's legal advisor, whom I once called "a double agent with cover blown from the start," because he was officially in the DHEW General Counsel's office; the late Wallace Rowe, a famous virologist, member of the RAC, and laboratory chief at NIAID; Betty Shelton, whose staff had a prodigious capacity for production of copy; and Maxine Singer, a Cancer Institute molecular biologist who had been in on the recombinant DNA controversy from the start, and whose contributions toward its resolution were both legion and indispensable. Burke Zimmerman, who joined us later on, brought with him the valuable perspectives of the environmental groups and of the congressional staffs.
7. Peter Hutt, letter to Donald S. Fredrickson, 3 March 1978, NIH Papers, Appendix A, pp. 239–56.
8. Donald S. Fredrickson, "A History of the Recombinant DNA Guidelines in the United States," in *Recombinant DNA and Genetic Experimentation*, Joan Morgan and W. J. Whelan, eds. (Oxford and New York: Pergamon Press, 1979), pp. 151–56.
9. Don K. Price, "Endless Frontier or Bureaucratic Morass?" *Daedalus* 107, no. 2 (Spring 1978): 75–92.

10. Donald S. Fredrickson, "Seeking Technical Consensus on Medical Interventions," *Clinical Research* 26 (1978): 116.

11. Maxine Singer, "Spectacular Science and Ponderous Process," editorial, *Science* 203 (5 January 1979): 9.

12. Office of the Director, National Institutes of Health, *Recombinant DNA Research; Documents Relating to "NIH Guidelines for Research Involving Recombinant DNA Molecules"*: Volume 1, February 1975–June 1976, DHEW Publication No. (NIH) 76-1138, August 1976, 602 pp. Volume 2, June 1976–November 1977, DHEW Publication No. (NIH) 78-1139, March 1978, 910 pp., and Supplement, *National Institutes of Health Environmental Impact Statement of NIH Guidelines for Research Involving Recombinant DNA Molecules*, Part One, DHEW Publication No. (NIH) 1489, 147 pp., and Part Two, Appendices, DHEW Publication No. (NIH) 1490, 438 pp., October 1977. Volume 3, November 1977–September 1978, DHEW Publication No. (NIH) 78-1843, September 1978, 936 pp., and Appendices, DHEW Publication No. (NIH) 78-1844, September 1978, 608 pp. Volume 4, August–December 1978, DHEW Publication No. (NIH) 79-1875, December 1978, 506 pp., and Appendices, DHEW Publication No. (NIH) 79-1876, December 1978, 456 pp. Volume 5, January 1979–January 1980, NIH Publication No. 80-2130, March 1980, 654 pp. Volume 6, January–December 1980, NIH Publication No. 81-2386, April 1981, 570 pp. Volumes 1–5 (5257 pages in all) are for sale from the Superintendent of Documents, U.S. Government Printing Office, Washington, D.C. 20402, and are available in approximately 600 public libraries of the GPO depository system. (GPO Stock No. for Vol. 1, 017-040-00398-6; Vol. 2, 017-040-0422-2, and two-part Supplement, 017-040-001413-3; Vol. 3, 017-040-00429-0, and Appendices, 017-040-00430-3; Vol. 4, 017-040-00443-5, and Appendices, 017-040-00442-7; Vol. 5, 017-040-00470-2.) Volume 6 is not available for sale.